LETTERS TO THE EDITOR.

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Osmotic Pressure and Ionic Dissociation.

No doubt there will be great joy in the camp of the ionists over the sinner that repenteth on seeing the letter of my friend Prof. Poynting, but I imagine that those who thought that he was about to break a lance on behalf of the opponent party will agree that he is but a fickle ally, if they do not go further and conclude that he has perhaps failed to understand the real question at issue.

All are agreed that Arrhenius and van 't Hoff and their satellites have rendered inestimable service by their generalisations, and the consequent application they have made of them; certainly the world has shown its esteem of their work. Moreover, there can be no doubt, as I stated not long ago in my presidential address to the Chemical Society, that in so far as *weak solutions* are concerned a law has been discovered which is broadly true *in mathematical form*: yet I have no hesitation in asserting that the fundamental premises on which it is based are destitute of common sense in the opinion of those who look at these matters without leaving chemical experience out of account; and I venture to think that this is not only their position, but also that of many physicists. Lord Rayleigh, in fact, in the course of his remarks on Prof. Fitzgerald's Helmholtz lecture at the Chemical Society in January last, actually said:—"It is to be hoped that chemists will take into grave consideration the emphatic warning that Prof. Fitzgerald has given, particularly as to the danger of supposing that there is any dynamical similarity between the condition of a gas and that of a dissolved substance in a liquid. . . . there is possibly a risk of pushing formal analogies too far, and of supposing that there is a real dynamical similarity, whereas perhaps there is only a similarity in mathematical law."

I for one require no better support than this, and shall continue to be, as I have been from the outset, a determined opponent of what, I think, may fairly be termed the nonsensical hypothesis of ionic dissociation, for there is no other appropriate term for a view which asserts that hydrogen chloride and a few other compounds are so loosely strung together that they fall to pieces when dissolved in water: out of sheer fright, it would seem, as no valid motive is suggested for such self-sacrifice; and no such charge of unprincipled levity of conduct is brought against the vast majority of compounds other than a few acids and akalies! I believe the view in question to be in entire opposition to the teachings of chemical experience; inapplicable to the explanation of the greater number of facts. It is a sign of the times that such views can obtain the credence that has been accorded to them; proof how little we care to criticise in these days when authority counts for so much—especially in Germany. On the other hand, the facts generally seem to be in entire accordance with an association hypothesis: and if Prof. Poynting would seriously devote himself to putting such an hypothesis into mathematical form, he would be rendering the one great service that is required of physicists in this connection. All that is needed, I imagine, is to show that the equations deduced from the one hypothesis are equally compatible with another diametrically opposed to it—surely a small thing to ask of mathematicians.

The dialectical skill of those who are seeking to impose on the scientific world the ionic dissociation hypothesis as the only true faith is very remarkable. They *must* be near relatives of Maxwell's demons, judging from the adroitness with which they extricate themselves from seemingly hopeless positions—no matter at what sacrifice, and the infinite elasticity of their views—which, indeed, are often so elastic that it matters little which way the argument turns. For example, in a recent discussion on accumulators at a meeting of the German Electro-chemical Society, the possibility of lead peroxide ions existing in solution having been suggested, it was urged that the presence of a few such would not suffice: whereupon Prof. Nernst said that this was no difficulty—it was only necessary to bear in mind the readiness with which silver was separated from cyanide solutions, in which the number of silver ions was inconceivably small. If so few will suffice, why not try to do without them altogether, one is tempted to say. As another instance, I may refer to Mr. Whetham's diplomatic action in at once offering to conclude a treaty of peace with Prof. Poynting by conceding the willingness of the ions, if necessary, to bear the water molecules on their backs or be chained to them as galley-slaves: formerly we were assured that ionic dissociation was purely platonic suicide, and that there were no Rhine maidens in the stream to attract the ions apart and bear them away. Of course, in preferring these terms, the effect is carefully left out of account which such copulation would have on the mystic charges carried by the ions; hypothetical burdens which place them on a level far superior to that of any commonplace independent molecule, serving to keep them in bounds, and which permit of subtle distinctions being made between ordinary and ionic dissociation—making the latter not dissociation at all, in fact, for ionists certainly ask both to keep their cake and eat it. Again, Mr. Whetham, I note, refers to ionic velocities. To me it seems impossible to believe that the ions are the infinite sluggards indicated in conventional time-tables such as those issued under Lodgian editorship—apparently with his approval; with all due deference to such authority, we must decline to accept so slow a service, the more if we are to believe that the gaseous and liquid states are in any way dynamically comparable. It may perhaps be argued that the charges act as brakes—but surely such an argument is double-edged, for if we are to suppose that the freedom of the ions be thus clogged, liberty can scarcely be theirs to act as ordinary gaseous molecules in effecting pressure.

I am free to confess that my condition of belief as to the existence of "atomic charges" and any form of ionic *dissociation* is purely agnostic—but this is clearly an attitude which is abhorrent to those who are now arrogating to themselves the position of superior persons to whom has been granted the mission and plenary powers to reform an ancient society long steeped in superstition: to wit, the chemists. But the chemists' chief concern has been, and still is, to establish facts, and by so doing, they have probably got nearer to the inner workings of nature than any other class of investigators: for such, the new inquisition has no terrors.

In a paper, of which the Physical Society—in a most aggravating and reprehensible manner—publish but the first two pages in the current number of their journal, Prof. Larmor tells us "that the facts of chemical physics point to electrification being distributed in an atomic manner, so that an atom of electricity, say an electron, has the same claims to separate and permanent existence as an atom of matter." But is not this view based on a particular—I venture to say, a narrow—interpretation of the facts of electrolysis? Do the facts of chemistry point to such a view? Those who cultivate chemical physics have a way of putting chemistry altogether aside, as a mere unimportant detail. Does it not entirely leave out of account the difficulties which the existence of so-called molecular compounds introduces? Do physicists—do chemists even—in any way appreciate these? I have already elsewhere contended that if there be an electron, it must be capable of acting piecemeal—no discussion of the question from this point of view has yet been attempted.

The facts must not be left out of account! There are many gifted mathematicians and physicists at the present day who are showing willingness to take such matters into consideration, but they are too prone to accept their facts at second-hand—often from those who, although nominally chemists, are destitute of *chemical feeling*—an indefinable instinct which, however, has a very real existence: consequently two parties are arising with no common ground between them.

Surely there is no need to be in so great a hurry—it is no disgrace to admit that we cannot yet explain all the mysteries of the universe. Lord Kelvin told us recently that he knew no more of electric and magnetic force, or of the relation between ether, electricity and ponderable matter, or of chemical affinity, than he knew and tried to teach his students of natural philosophy fifty years ago in his first session as professor. And it should also not be forgotten that one to whom all yield respect has written: "It is extremely improbable that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric currents, and so become independent of these provisional theories." (Clerk Maxwell, "Electricity and Magnetism.")

Meanwhile, do not let us pervert the morals of our student youth by talking glibly of atomic dissociation, and using a dogmatic phraseology which leads them to believe that we have

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reached finality and gives them totally false ideas as to what *we know*. It is so easy to make use of all that is good in the new work by substituting a neutral phrase, such as coefficient of activity, for coefficient of dissociation—for all that we have really done is to recognise that certain compounds exercise a superior degree of activity, and to measure the relative degree of activity of these. To substitute for common-sense expressions which all understand—and even to promulgate at County Council expense—a set of shibboleths which commit us to a definite hypothetical interpretation of the facts is unnecessary, undesirable, and unscientific. Such metaphysical speculation is obviously doing the deepest injury to the cause of exact science.

Henry E. Armstrong.

On the Publication of Original Work.

Much has been written and said as to the facilities for the publication of original researches in this country. It is now becoming quite a regular custom for English comparative anatomists to publish their work in a foreign journal. Not only the morphologist, but also the systematist has found this necessary. One is naturally led to inquire why—with so many learned and wealthy societies in our midst—publication cannot be effected so as to give the author the necessary printing and illustration in a style comparable with that of continental journals, and with a minimum amount of delay?

In this country the only sources of publication for monographs on zoological subjects are the *Phil. Trans.* of the Royal Society, the *Trans. Zool.*, and the *Trans. Linn.*, and of these it would naturally be supposed that the *Trans. Zool.* is preeminently the place for such publications. But it will scarcely be credited that a wealthy Society like this, for some unknown reason, should allow in some cases as long as two-and-a-half years to elapse before publication of material received. Thus I find, on referring to volumes of the *Trans.*, a paper received November 1, 1892, read December 20, 1892, was published in February 1895. Another, received December 5, 1892, read February 14, 1893, was published October 1895. Still another, received October 14, 1893, read November 7, 1893, was not printed till April 1896! These are examples chosen at random.

There seems no obvious reason why any or all of these should not have been published within six months from the date of reception. A fourth instance, which I here wish to narrate, will, I trust, serve the purpose of showing zoologists the need of some more speedy means of publication.

In the winter of 1894–95, I completed a piece of work on the suprarenal capsules in fishes, and was advised to offer it to the Zoological Society for publication. The paper was received, in the first instance, on June 6, and I hoped it would have been taken as read at a meeting of the Society held in that month. It was, however, not read till November 19, when Prof. Howes was good enough to undertake it for me. It was ordered for publication in the *Trans.*, and now (November 14, 1896), nearly twelve months from the date of reading, I have not yet received my proofs. Surely such extraordinary delay as this ought not to be necessary.

During such a long period I have found it necessary to keep pace with much literature bearing upon the subject; but more than this, I have just suffered the chagrin of seeing a paper embodying a large slice of my results published by an Italian journal.

Perhaps some others will be found to agree with me that some means ought to be found of getting earlier publication in comparative anatomy and allied subjects. In the minds of many, I feel sure, there can be little doubt that the Zoological Society should undertake such work.

I do not wish to make out that I have been treated exceptionally, or in any way unjustly. My experience has been no worse than that of many others. The officers of the Zoological Society have treated me with every courtesy, and have even allowed me to publish an abstract of my paper elsewhere. But, nevertheless, I fail to see why the work could not have been published within six months from the time of reception.

Swale Vincent.

Mason College, Birmingham, November 14.

Cultivation of Woad.

As supplementary to the article on the cultivation of woad, by Messrs. Darwin and Meldola, in Nature for November 12, it may further be stated that this plant has been grown besides at Parson Drove, at Boston, Wyberton, and Algarkirk, in the Lincolnshire Fenland, for a very long period. An account of its cultivation, with details of the process and preparing it for use, will be found in Arthur Young's "Agricultural Survey of Lincolnshire," published at the end of the last century. A more modern account will be found in "The History of the Fens of South Lincolnshire," recently published. This plant is not cultivated in any other part of England than the Fenland, and the total area grown altogether yearly does not, as a rule, exceed fifty acres. It requires very good land for its cultivation, and much rich old pasture land has been broken up for the purpose, for which as much as 10*l.* an acre has been paid for rent, and 150*l.* to 200*l.* for purchase of the freehold. The price obtained for woad was formerly about 25*l.* a ton, but it has declined in recent times to 9*l.* or 10*l.* The woad, when prepared for market, is not used for dyeing, but is mixed by woollen dyers with indigo to excite fermentation and fix the colour.

Boston. W. H. Wheeler.

With reference to your article on "An English Woad Mill," may I mention that Billingsley, in his book, published in 1798, on "Agriculture in the County of Somerset," mentions woad as an important article of cultivation, raised principally in the neighbourhood of Keynsham, near Bath. The mode of preparation, described by Billingsley as in use one hundred years ago, closely resembles the description given in Nature as in use at the present day. He adds that the crop is a profitable one; so lucrative, indeed, that few farmers who can raise it, ever discontinue the practice. He also mentions that it was cultivated by one Harvey, more generally known as the "Woad-man," at a farm near Mells. The cultivation of woad does not appear, therefore, to have been so very rare in the last century; but whether it is still cultivated in Somersetshire, I am unable to say.

Rosa M. Barrett.

Kingstown, Dublin, November 14.

"X-rays with a Wimshurst Machine."

There is an error, for which I am responsible, in my letter of July 24 (p. 31). The words kathode and anode should be interchanged in one sentence, which should then read thus:—"The same reasoning would indicate that it would be well to make the *anode* convex towards the *kathode*, and fairly small. . . ."

Eton, November 13. T. C. Porter.

FLYING BULLETS.

Quite recently M. Tissandier, editor of *La Nature*, received from Prof. Mach, formerly Professor of Physics at Prague, now Professor of the history and theory of inductive science at Vienna, a letter containing a photograph of a bullet in motion (Fig. 1). The photograph was taken by Prof. Mach's son, and shows most clearly the waves of air caused by the bullet's passage through the atmosphere.

M. Tissandier, wishing for an explanation of the experiment and description of the apparatus, wrote to Prof. Mach, and received the accompanying diagram (Fig. 2), with the following short account. "My son took the photographs of the bullet by using a spherical silvered-glass mirror. M M is the mirror, P the bullet, E the screen, B the photographic apparatus, S the spark. The bullet causes a sonorous wave, by which the Leyden jar is *mechanically* discharged, and produces the spark S."

It may be added that the description of his first apparatus appeared in *La Nature* of 1888.

Our readers are also familiar with the photographs of flying bullets which were exhibited at the soirée of the Royal Society in May 1892. These were results of experiments made by Mr. Vernon Boys, obtained by a modification of an old method. One slide showed the small pieces of paper scattered by the bullet passing through a sheet, and these were carried on in the same direction as the bullet itself; whereas in the case of a magazine rifle bullet going through a sheet of glass, the shattered pieces of glass appeared to travel in an opposite

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